attention to the subject and either confirm or disprove facts which, if true, would seem to afford a crucial test of the truth or falsehood of some of the most important theories of modern geology.

The fact which I assert is, that there are no traces of glacial action, or of raised sea-beaches in Orkney.

I speak from an intimate personal acquaintance with these islands, which are my native county, and almost every yard of whose surface and shores I have explored with rod and gun, and in the course of canvassing at elections, and for many years back keeping a special eye on this very subject. Now I can assert positively that I never saw a boulder or perched block, or the trace of any till, boulder clay, kame, eskar, raised beach, or other form of glacial or marine action.

The whole of the islands consist—except a small patch of granitic axis—of Devonian strata, bare in places, but for the most part covered with a mantle of soil, which is the obvious result of the disintegration of the subjacent rock by existing subaërial causes, such as wind, frost, and rain.

In places, where soft strata come to the surface, this soil is deep and clayey, and the sections of it, afforded by the coast-line, might readily be mistaken at a distance, or by a superficial observer, for boulder-clay. But a close examination will show that the stones in this stony clay are always angular and always similar to the adjoining strata, and that the larger stones are generally deposited, allowing for subsidence and displacement, in the original lines of stratification conformable to those of the unworn rock below them. A good example of this may be seen within 200 yards of Kirkwall, on the east side of the bay under Cromwell's old fort.

Let any one compare this with the section of glacial boulder-clay shown on the other side of the Pentland Firth at Scrabster, and he does not require to be a geologist to understand the difference between a surface soil of glacial deposit and one of disintegrated rock.

In like manner I have observed innumerable sections of surface soil and of mounds and ridges, which at first sight might have passed for marine or glacial, and I have invariably found them to consist of angular fragments of the subjacent rock passing on the one hand into thoroughly decomposed rock or soil, and on the other into the solid strata on which they rest.

I believe I may state broadly that there is not a rolled or rounded stone or pebble, or trace of sand or gravel, in all Orkney above the level of the present sea-beach and blown sands, and away from the beds of the existing lakes and small streams.

There is not the vestige of a raised beach along the hundreds of miles of rocky coast of the various sounds and islands, or in the many sheltered inlets where, in the nearest counties of Scotland such as Sutherland, Ross, and Cromarty, raised beaches are invariably seen. All recent movements seem to have been movements of subsidence and not of elevation. The Loch of Stennis, with its surrounding plain, affords conclusive proof that at no recent geological period can the level of the sea have stood higher relatively to that of the land than it does at present. Had it done so the Loch of Stennis, which is now exactly level with the sea so that the tide flows into and out of it, must inevitably have been a sheltered inland fiord of salt water extending to the hills which bound the plain, which as the land rose or the sea retreated, must have left the plain covered with sand, shingle, and marine or brackish shells, of none of which is there the slightest trace, but, on the contrary, the ordinary rock strata with their disintegrated surface soil, occupy the whole plain and come up to the margin of the existing loch.

Now as to the inference from these facts.

The received theory of most glacialists is, that during the glacial period there was a great polar ice-cap extending over the whole of Scandinavia, Scotland, and a great part of England and Ireland. As a corollary of this many draw the inference that such an accumulation of ice, by displacing the earth's centre of gravity, would raise the level of the sea in the Northern hemisphere, and thus account for the higher levels relatively to the land at which it has undoubtedly stood.

Others contend that the glaciation was more limited and only extended in islands as it were, round each considerable mountain group in northern latitudes, and these attribute the phenomena of raised beaches, &c., to local elevations of the land rather than to general elevation of the sea.

Now here appears to me to be an opportunity of applying the *experimentum crucis* to these two conflicting theories.

If it be true that Orkney is not glaciated, and has no raised beaches, it seems to follow that the second, and not the first, of these theories must be the true one.

The second theory would account perfectly for the boulder-clay being found in Caithness, over the plain of which we may easily suppose the glaciers from the great mountain range which bounds it on the south and west, to have extended as far as Scrabster and the south shore of the Pentland Firth, while in Orkney there were no glaciers, because there was no great local mass of mountain region to produce them.

But, on the theory of a great ice-cap, I cannot see how Orkney could fail to have been planed by ice and covered by boulders, perched-blocks, and masses of glacial clays, sands, and gravels.

In any case the absence of raised beaches and of all traces of marine action above the present sea-level, seems to be inconsistent with any theory of a general and uniform rise of the ocean in these latitudes.

As regards the Shetland Islands I cannot speak with the same confidence, not being so intimately acquainted with them; still, having travelled over a great part of the principal islands, and coasted along their shores, I can assert that I have never seen any traces of glacial action, or of raised beaches. The latter must, I think, inevitably have shown themselves in the form of sea-caves at a higher level, such as those at Cromarty, had they ever existed, as the present line of exposed rocky coast is worn by the waves into innumerable caves and clefts.

As to boulders or boulder-clay, I do not believe they exist, and the only rounded or water-worn stones I have ever seen have been rolled in the Devonian and not in any modern seas, and result from the surface disintegration of the great conglomerate.

These are abundant in exposed situations, and they show the necessity for care in inferring modern glacial or marine action from the presence of rolled stones of foreign rocks.

In conclusion, I believe that these groups of islands, Orkney and Shetland, have never been subjected to glacial action or submerged and subsequently elevated, in any recent geological period, and that these facts are inconsistent with any theory of a great polar ice-cap, or of any uniform rise of the level of the ocean in northern latitudes. S. LAING

Brahan Castle, Dingwall, N.B., August 25

Meteorological Effects of Eclipses

IN connection with certain variations of temperature observed during the total eclipse of the moon on August 24, 1877, by M. Berigny, and discussed at a late meeting of the French Academy of Sciences, as reported in NATURE (vol. xvi. p. 412), I am reminded of some observations made on board H.M.S. *Challenger* during the total eclipse of the sun on April 6, 1875. The position of the ship at noon of the day of the eclipse was in lat. 27° 13′ N., long. 137° 59′ E. about 400 miles south of Japan and 200 miles due west of the Bonin Islands. If my memory be correct, the eclipse was only partial for the part of the world we were in, a portion of the sun's disc being still visible in the shape of a thin crescent at the moment of maximum obscuration. The eclipse, occurred in the afternoon, and was heralded by a breeze from the south-west, which continued during the rest of the evening; but what at the time struck us as very remarkable was the fact that it was accompanied by a rise of the surface temperature of the sea, as will be seen from the following observations made at the time:—

April 6, 1875.	Temperature of sea-surface.	
4 A.M.	20·3° C.	
10 ,, ...	20·9 ,,	
Noon to 3 P.M.	21·1 ,,	Time of the eclipse.
4 ,, ...	22·5 ,,	
5 ,, ...	22·2 ,,	
6 ,, ...	21·9 ,,	
7 ,, ...	20·9 ,,	
8 ,, ...	20·0 ,,	

The *Challenger*, progressing at the rate of about three knots per hour, had just entered an area of alternate streaks of warm and cold water, the former due to the North Pacific equatorial current, known as the Kuro-Siwo or Japan current, the latter to the Arctic current which flows down off the east coast of Nipon, so that the observed rise of temperature, and perhaps also the south-westerly breeze which sprung up at the commencement of the eclipse may be a mere coincidence, and I give the observations for what they are worth.

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During the discussion in the French Academy the theory suggested by M. Berigny that an eclipse of the moon might have an appreciable effect upon the temperature-conditions of our atmosphere, namely, by cooling the latter, does not seem to have met with much encouragement. It would be more extraordinary if such a phenomenon as a total eclipse of that luminary were found to exercise no disturbing influence of any kind upon the terrestrial atmosphere during the time of its occurrence.

J. J. WILD

The Development of Batrachians

IN reference to the article in NATURE (vol. xv. p. 491) on the development in certain instances of Batrachians without metamorphosis, Mr. B. G. Wilder writes to the *American Naturalist* (vol. xi. No. 8, August, 1877, p. 491) to point out that the author of the article in NATURE has overlooked Prof. Wyman's observations on the development of *Pipa americana*, published in the *American Journal of Science and Art* for 1854 (ser. 2, xvii. pp. 369-374).

Wyman has there stated that the eggs of *Pipa* are transferred by the male to the back of the female, which presents "a uniform surface throughout." "Their presence excites increased activity in the skin, which thickens, and is gradually built up around each egg, so as at length to inclose them in a well-defined pouch." On pages 370 and 371 he figures and describes the earlier embryos as having "three branchial appendages on each side of the head." "In a later stage the external branchiæ had disappeared, but a small branchial fissure was detected on each side of the neck, and within this on each side a series of fringed branchial arches."

In endeavouring to obtain some confirmation of Prof. Wyman's observations, Mr. Wilder suggested an examination of certain eggs of this singular Batrachian preserved in the Warren Anatomical Museum of Harvard University. The examination was made by Mr. C. S. Meriot, who reported as follows:—

"I have examined two eggs from the back of the *Pipa*, and found the embryos a little more advanced than that figured by Prof. Wyman; they are between 12 and 13 mm. in length. The gills were partly absorbed, but a single slit with the gills still projecting could be readily seen on each side at the back of the head. I could not make a more detailed examination, as the eggs were not well enough preserved."

It would therefore appear that Dr. Peters' remark (which was translated in the above-mentioned paper in NATURE on this subject) that "no one has detected branchiæ in the embryo of the Surinam toad" is not well founded. It would be very desirable, however, to have further observations on this interesting subject made, as likewise on the development of *Hylodes martinicensis*, which was the principal subject of the former communication to NATURE.

P. L. S.

Notes on the North East Australian Monotremata

ENGLISH naturalists seem to be "all abroad" on this subject, judging from some remarks in NATURE, vol. xv. p. 257.

P. L. S. states his conviction that Tachyglossus will be discovered in the ranges of N. Queensland when these have been properly examined, being evidently quite unaware that both Tachyglossus and Ornithorhynchus have been known to inhabit the northern part of this colony for several years, and that a discussion has been carried on for some months in the columns of the *Queenslander*, on the "Generation of the Echidna and Platypus," between Dr. Bancroft and Mr. Bennett. I merely allude to this subject to give English naturalists the latest discoveries made by Dr. Bancroft in his researches into this more than usual *prickly* subject.

Dr. Bancroft dissected a female specimen and found a quantity of milk in the stomach. He was unable to find any mammary glands, and came to the conclusion that the mother Echidna fed its young by regurgitation. This theory was combated by Mr. Bennett.

On dissecting a second female Dr. Bancroft discovered the mammary glands, but not like those of any other known mammal, for the nipples were inverted, the cavities thus formed being protected by stiff bristle-like hairs.

The young Echidna has to thrust its bill into the cavity to obtain its food. This is a curious adaptation to the wants of an animal, for it is certain, from the curious formation of the head and rostrum, that it would be impossible for the young to suck a nipple; as it is the forcing in of the bill expresses the milk which it is enabled to suck in when lying in the bottom of the cavity.

I secured a specimen of an adult female having a fine healthy young one in the pouch, and preserved both. On opening the stomach I noticed a quantity of a white substance which seemed to be inclosed in a thin membrane. I did not know what this was until I read Dr. Bancroft's letter on the subject, but unlike him I arrived at a different solution of the puzzle. Instead of the females sucking themselves (the way in which Dr. Bancroft accounts for the presence of milk in the stomach), I believe that, after a certain time, when the spines begin to grow on the young Echidna, the irritation causes the mother to take it out of the pouch, and to feed it by regurgitation, until its spines are sufficiently grown to protect it from its enemies. I was led to this belief by discovering a nearly adult male with no sign of any food in its stomach except milk.

I was encouraged to dig this individual out of its stronghold, by noticing several unusually large tracks going in and out of the burrow. These, undoubtedly, were the footprints of the female when she came to feed the young. The spines on this specimen were not at all stiff, especially at their bases, and would have offered little or no protection against the attacks of a native dog or eagle. This may seem a wild proposition, but I have only adopted it after mature thought, and observing four different females.

We must adopt some explanation to account for so strange a phenomenon, unless we jump the question by allowing that the females of Tachyglossus suck themselves, a most unlikely proceeding on their part, and a solution to the difficulty as unsatisfactory as it is improbable. We have only to guard against chronicling false facts which seem to me to be the greatest enemies which science has to contend with, and we are not so apt to go wrong. Mistakes are often made, but invariably meet with a correction, and should the above solution to a difficulty which has taxed, and is still employing, some of our wisest heads, prove wrong, I shall be only too glad to accept the correction, thankful that my blunder has assisted in rightly solving an interesting and vexed question. The blacks inform me that the Echidna lays a white egg, and the Platypus a black one, which are hatched in the abdominal pouch.

I trust that some of your many scientific contributors will come to our rescue in solving this point. W. E. A.

The late discoveries by Goldie, D'Albertis, and MacFarlane, have produced a number of forms (botanical) identical with those obtaining in N.E. Queensland, thus further proving the original fundamental unity of the two countries. The Australian Dingo ought to be found in New Guinea as also our tiger cat.

W. E. A.

On Time

I HAVE deferred my answer to the remarks of Mr. J. J. Murphy (NATURE, vol. xvi. p. 182) till now, in order to see if my letter of June 14 would cause more discussion. But it seems that my views are not deemed worthy of much consideration in England; I shall therefore say only a few words in conclusion here, but hope to take up the question elsewhere.

Mr. Murphy thus summarises a part of my letter:—"The postulate that a velocity, *e.g.*, that of the earth's rotation continues unchanged, is arbitrary, incapable of proof, and justified only by practical convenience." I should wish that he had added, "and to be settled by definition." Mr. Murphy goes on to say, "It seems to me, on the contrary, that the postulate is not necessarily arbitrary but may be absolutely justified by fact." I do not believe there is a great difference between "justifying something by practical convenience" and "justifying it absolutely by facts." Perhaps, in the opinion of Mr. Murphy, the constancy of the velocity of the earth's rotation is proved by the pendulum. But it is universally admitted that the pendulum is controlled by the earth and not the earth by the pendulum.

Mr. Murphy, in admitting that he sees no way of proving that the force of gravitation continues unchanged, acknowledges that in natural philosophy many things are taken for granted which call for closer consideration, and this was precisely my motive for writing my letter.

Though Mr. Murphy does not agree with me in all points I am thankful for his remarks. V. A. JULIUS

Breda, Holland, August 29

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